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fined to the medical schools. Here both have been treated too largely as applied sciences. Both would greatly profit in being taken from the medical schools and established, like physics or chemistry, in separate institutes where both the pure and applied science should be taught. The biochemical laboratory should be one of the laboratories of the university, just as the laboratory of experimental physiology, or organic chemistry. It should be in the hands of investigators, and should give instruction not only in urine analysis, but in the principles of metabolism. For the purpose of mutual helpfulness it should be in close connection with the laboratories of experimental physiology and organic chemistry. It is greatly to be hoped that the progress of this science in America may be furthered by the establishment of professorships of biochemistry and of an American Journal of Physiology and Biochemistry to provide a ready means of publication for physiological and biochemical papers.

THE POLYPHYLETIC DISPOSITION OF LICHENS.[1]

BY FREDERIC E. CLEMENTS.

The present trend of thought upon the morphology and disposition of the lichens must be most encouraging to those, who stood at first alone, and then with ever-increasing company, for the complete acceptation of the Schwendenerian hypothesis, and of the morphologic and phylogenetic theories involved in it. Even during the present decade, botanical literature has not lacked for articles, penned chiefly by lichenologists, disproving in its entirety the algo-lichen theory, and maintaining the autonomy of the lichens, as they are pleased to term it. When the "symbiosis", "consortism," or parasitism of lichens was established beyond a doubt, and polyphylesis was postulated as a necessary consequence, the lichenographers again rose en masse, arguing and pleading for the dignity and autonomy of their group. Since the tacit and universal acceptance of the polyphyletic origin of the lichens, opposition to their distribution among the other fungi had practically ceased, until the revival of the question by Reinke's articles published during the past year or two.

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[1] Read before the Botanical Seminar of the University of Nebraska. December 5, 1896.

To the Seminar, which has stood since its inception unqualifiedly for the Schwendenerian theory, and for the consideration and treatment of all fungi as fungi, my task will seem a gratuitous one. It may not be, however, entirely unprofitable to consider in detail the arguments still advanced by some botanists against what is here regarded as the ultimate disposition of the lichens.

In addition to Reinke's rather exhaustive papers, Gregory and Schneider have written short articles, chiefly recapitulatory of Reinke's views, and hence of little import, were it not for the fact that the first endeavors to · throw the weight of Schwendener's half-expressed disapproval to her side of the question. Reinke's articles, however, form the rallying ground of all those fearful of the degradation of the autonomous dignity of the lichens, and will alone be discussed here. His conclusions are based upon serious work, and, in consequence, deserve earnest consideration, although not infrequently ridiculous to one free from the trammels of "consortism," "mutualism," etc.

The greater portion of Reinke's arguments are given in his second paper (Pringh. Jahrb. 26 : 524, 1894). In the prefatory remarks to his fifth article (Pringh. Jahrb. 29 : 171, 1896), which treats of the natural lichen-system, he adduces certain general arguments that have equal weight on either side, and makes some specific objections that have little relevancy and less significance. In consequence, the following discussion will be limited chiefly to the second paper, the more important points of which will receive successive treatment.

"Concerning ascolichens, it may be postulated that the component fungus no more exists, probably never existed, in the free state." Since lichens are lichens, and, according to Reinke, every lichen a "consortium," whether its fructification be by means of asci or by basidia, he begs the question in limiting his statement to ascolichens. In complete refutation of this, I might then cite Moeller (Flora 77 : 253, 1893), who shows conclusively that the thelephoroid fungus represented by the three lichen genera, *Cora*, *Dictyonema*, and *Laudatea*, may be, at the same time, a saprophyte, or a parasite upon two different genera of algæ, *i. e.*, a facultative parasite. For the ascolichens, Reinke contradicts his own statement that their fungal prototypes no longer exist (Pringh. Jahrb. 28 : 71, 87, 135, 1895). The mere fact that he has been obliged to divide the genera *Calicium*, *Bilimbia*, *Bacidia*, *Placographa*, *Melaspila* and *Arthonia* into a series of lichen genera corresponding respectively to the above, and a series of " myco-genera," *Mycocalicium*, *Mycobilimbia*, *Mycobacidia*, etc., solely because certain fungi, having parasitized accidental algal cells, have lost their saprophytic habit, is conclusive. It is also admitted by the best mycologists that *Biatoridium*, *Lecidea* and *Buellia* can be distinguished from *Biatorella*, *Patinella*, and *Karschia* only through what can be called scarcely more than incipient parasitism. More than this, *Buellia myriocarpa*, as Reinke himself admits (Pringh. Jahrb. 28 : 93, 1895), is sometimes provided with gonidia, sometimes lacks them. It is then either a fungus or a lichen. This species is certainly a remarkable one, in that it belongs to two different and distinct subkingdoms, or at least classes! Were cumulative evidence needed, the repeated artificial synthesis and analysis of lichens would be more than sufficient to discredit Reinke's statement, notwithstanding his inability to appreciate the weight of these facts.

It is a scientific truism that phylogeny is the science of probabilities. No thoughtful scientist should dream of demanding absolute proof in general phylogenetic problems. In discussing questions of phylogeny, when the ultimity of probability is reached, except in the rarest cases, argumentation must cease. As for hymenolichens, we may regard Moeller's careful researches as, comparatively speaking, absolute proof of their direct and recent derivation from the *Thelephoraceæ*. For the ascolichens, enough has been said to demonstrate the conclusion toward which extreme probability points.

" The attempts at the distribution of the lichens have been of an unsatisfactory nature." It is by no means true that, to know the need of a reform, one must know how to bring such a reform about. If the method of reformation, or of rearrangement is hard to discover in those matters in which the factors are well-known and can be carefully weighed, how much more difficult is it where phylogeny with its undiscovered and undiscoverable quantities complicates. May we not, then, be pardoned if we consider Reinke's criticism of Von Tavel's endeavor to distribute the lichens as essentially puerile, and of negative weight in serious discussion?

"Taxonomy should, moreover, follow practical lines." It has been generally supposed that the constant endeavor of taxonomy during the past century has been to rid itself of the burden of practicality. If, as Reinke suggests, attempts to discover the phylogeny of any plant-group must always be unfruitful to a great extent, then a practical system, not a theoretical one, is a desideratum. If this is true, it is both remarkable and unfortunate that the best botanical effort of decades past has been directed to the replacement of artificial systems by natural ones, and to the improvement of the latter. As a matter of fact, such is not true. Taxonomy is, ultimately, never a means, but an end, and the demands of practicality in a system are entirely without force, until those are fully met which are entailed by the departments of botany that true taxonomy should summarize. Thus, the rapidity with which a system may be leaved over, is no index of its value. In fact, natural systems are necessarily complicated, and great convenience and "usability" in any system are in themselves suspicious. Reinke laments the fact that the lichens have disappeared so suddenly from a prominent place in texts, that it often involves trouble to find them in "anhänge" to different fungus-groups. Likewise, it would require less manual labor if the lower cryptogams were still grouped as algæ and fungi, and, from the same point of view, it is a great bother to follow an apetalous-family into some obscure nook among the *Choripetalæ*. Yet there are those who prefer an expression of probable phylogenetic relationship in a system to mere utility, and, who, unlike Reinke, have no "misgivings as to the fitness of arranging a great plant-group, so rich in forms and numbers as the lichens," in "anhänge," until a better disposition is possible.

The lichenologists, the specialists in this branch, "have intuitively opposed the "sidetracking" of the lichens." I mention this argument of Reinke's merely to show the peculiar cogency so characteristic of his article. It would not have been inopportune to cite the particularly felicitous, intuitive opposition of lichen-specialists to the Schwendenerian theory. But the author, naively, states that, while he has sympathised with this later stand of lichenologists, it was unfortunate that their weapons were directed against the Schwendenerian theory.

"In the form and structure of their vegetative organs, the lichens are closely connected with the other green, chlorophyll-bearing plants." If it were worth while to discuss this question, it might be readily shown that the only resemblance in form is a superficial one, due to parallelism, while the other great similarity arises from the fact that the one possesses assimilative power in and of itself, while the other appropriates that inherent in another organism. It is useless to press this point, however, since it is directly dependent upon whether the lichen is regarded as a instance of parasitism, or of "consortism."

Reinke, apparently, labors constantly under the delusion that those who contend for the distribution of the lichens, deny their polyphylesis. Contrariwise, they were the first to postulate and to establish it. All are in accord that, while the lichens originated from the fungi polyphyletically, this origination occurred at comparatively few points, and that the modification and specialization of these phylogenetic lines took place after the fungus had become parasitic upon an alga. No one who traces the lichens back to the fungi would for an instant maintain that each lichen family finds its prototype among the fungi. But, on the other hand, one must insist that at those points where fungi passed into lichens, an almost perfect series of gradations is noted, and that phylogenetic and morphologic continuity are complete at these places.

Naturally, the chief argument of the author is that the lichen is not a parasite, but a "consortium." In direct contradiction to this, as Reinke himself admits, is De Bary's statement that the fungus occasions considerable change in the host, the alga. Reinke is unable to evade this, but prefers to interpret it as an advantageous adaptation due to the synthesis of fungus and alga to form a "consortium." Then, in like manner, the increased rapidity of division, and the great distension of the cells of an elder leaf, occasioned by the presence of æcidial filaments, are adaptations traceable to incipient "consortism," if you wish, parasitism, in reality. The mere fact that the histogenetic relations of the cells in a tissue are not such that continuous multiplication of cell-individuals is possible, as is the case with free algæ, has no significance. The tissue-cell and the free alga exhibit essentially the same biological behavior when parasitized. The only difference is that, in the one case, cytogenesis fixes a limit, in the other, it does not.

In many cases, moreover, parasitism effects little or no change in the tissues. This is notably the case in Erysipheæ, in many Peronosporaceæ, and Uredineæ. On the other hand, algal cells often become so completely involved in hyphal threads that division is impossible, when the existence of real parasitism is quickly demonstrated.

The significance of the soridium and of hymenial gonidia is regarded by Reinke as very considerable. To me, both, but the soridium especially, are mere modifications of the fungus due to changed habit, and contingent upon incipient or advanced desexualisation. The significance of either, were they universally present, would be neither profound, nor otherwise inexplicable. The fact that they occur almost wholly among forms possessing a thallus of a considerable degree of development, and the fact of their utter absence in the primitive types is sufficient proof of their lack of meaning. Of equal weight is the presence of lichenin. The elementary condition of our knowledge of microchemistry should deter anyone from generalizations derived therefrom. Reinke's statement concerning the characteristic presence of many chemical substances is essentially one of the antiquated arguments of the lichenologists, and is scarcely more valid than that one in which, speaking of the discriminative powers of certain genera of insects, which feed upon lichens, and not upon fungi, Cooke says: "These insects must have come to a sounder conclusion than some men, viz., that lichens are not fungi with the addition of an innocuous green alga."

The most curious error of Reinke's is made in explanation of Moeller's researches upon *Hymenolichenes.* The author says frankly that after reading Moeller's article he asked himself if its conclusions were contrary to his opinions. At the same time, he affords an instructive illustration of how the interpretation of a fact may entirely controvert its meaning. If one were to find a facultative parasite growing at the same time on the ground, and upon two different host-plants, the explanation would be so obvious that one would not waste a thought upon it. The fact that the saprophyte exists alongside the parasite, the lichen, would mean simply that here was the starting-point of a phylum. Not so with Reinke. To him the occurrence of *Cora* as a facultative parasite signifies that here is a fungus, which forms "consortiums" with different algæ, a proof, as he takes it, that the fungus alone is insufficient for the thallus-body. Also, a proof in my mind that a host is indispensible to any parasite. As for the occurrence of hymenomycete and hymenolichen together, Reinke considers this entirely analogous to the concomitant appearance of alga and lichen, and, as I have pointed out above, this is true, since it involves nothing more than the simultaneous occurrence of a saprophyte, its host, and the same saprophyte in the role of a parasite.

I have taken up the above arguments in detail merely to demonstrate the little bearing they have upon the question. In determining relationships, phylogeny alone has value; histogeny, morphology, and physiology are useful only so far as they can furnish data respecting the probable phylogeny. I have already said enough to show that I appreciate the exceeding difficulty of retracing phylogenetic results. Happily, in the lichens, the phyletic lines are comparatively short; this is especially true in the hymenolichens, where the developmental processes are going on before our eyes. On the one side is the indubitable fungus ancêstor, the thelephora; on the other, the two direct descendants, *Cora*, and *Dictyonema*, incipient phyla, whose future directions of development are foreshadowed in the *Laudatea*-forms.

The ascolichens were derived from the *Ascomycetes* at an earlier period, and their great specialization and the interval of time elapsed have to a considerable degree obscured the points of departure. But, as has been pointed out above, the " fungo-lichen " genera, *Calicium, Bilimbia, Bacidia, Placographa, Melaspila, Biatorella-Biatoridium, Patinella-Lecidea, Buellia-Karschia, Graphis-Hysterium,* and the species, *Buellia myriocarpa,* leave little doubt concerning the derivation of their corresponding families. They simply await the thorough investigation given hymenolichens by Moeller. If we are to consider only the highly specialized ends of phylogenetic branches, we should all be constrained to admit that lichens must receive treatment as a group. But, in such an event, we should do the greatest violence to phylogenesis as the determinant in taxonomy, in making lichens coördinate with algæ and fungi, merely on the basis of a physiological character. I admit that there could be a class, or a branch, *Lichenes,* based upon this physiological character, or perhaps upon morphological characters induced by adaptation to assimilation-processes. So, also, might there be a group, *Parasiticæ,* coördinate with *Phanerogamæ,* which would include such closely related genera as *Cuscuta, Razoumofskya, Phoradendron, Thalesia,* etc. Such is the logical result of Reinke's opinion that a more or less constant physiological character, for lichens, " consortism," obtains for the delimitation of great groups, in spite of the significant evidence of phylogeny.

Summarizing: Reinke's conclusion that lichens are physiologically and morphologically distinct from fungi is untrue, and his statement that it is impossible, on account of certain physiological characters, to distribute them among fungi is equally unwarranted.